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Hidden Variables and Quantum Uncertainty
(Written Symposium, 7th Issue)

Variables cachées et indéterminisme quantique
(Symposium écrit, 7ème livraison)

Verborgene Parameter und Quanten-Unbestimmtheit
(Schriftliches Symposium, 7.Heft)

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Contents | Sommaire | Inhalt

Nous avons aujourd'hui le plaisir de présenter un article original de John S. Bell, en réponse à quelques objections. Nous serions heureux si les auteurs concernés (de Broglie, Lochak, de la Peña, Cetto, Brody) voulaient bien utiliser nos colonnes pour continuer la discussion ainsi engagée.

Nous profitons de l'occasion pour signaler un article de Flato, Piron, Grea, Sternheimer et Vigier intitulé "Are Bell's Inequalities Concerning Hidden Variables Really Conclusive?" (Helv.Phys. Acta 48, 219-225 (1975)). La principale objection faite est que les inégalités de Bell ne jouissent pas des propriétés souhaitables de stabilité:

"As we shall see later, we have to deal with a continuous hidden variable, λ, a continuous density, ρ, which in the local case is a function of λ only, and an observable (spin or helicity direction), A, taking only the values $\pm$ 1, which in the local case depends only on λ and on the orientation of the corresponding measuring apparatus. Now the measured quantities, and also the inequalities they satisfy in the local case (Bell's inequalities), can be made stable with respect to ρ. This means, as can be easily seen, that small non-localities in ρ (dependence on other parameters than λ for suitably regular ρ's) will cause only small changes in Bell's inequalities.

However, since the quantity A takes only the discrete values ± 1, there is no sense in which stability under non-local deformations of A can be guaranteed: one cannot have, in this connection, any 'almost local' theory 'close' to the local one. Now, as we explained before, A is not even a measured quantity. Locality is already quite a strong assumption, but since stability does not exist here, even if one will be able to measure quantities like A(λ) in the future, how can one ensure no experimental contamination of locality which would make our 'local predictions' completely useless?

Moreover, if we choose our models at random, for any type of non-locality, almost every model will give predictions different from the local case, and we can choose models of $\rho(\lambda)$, $A(\lambda, a, b)$ (which depends a priori at random on b), etc..., such that we can have a maximal deviation from the local case."

14.1 J. S. Bell - Locality in Quantum Mechanics: Reply to critics

The editor has asked me to reply to a paper, by G. Lochak, refuting a theorem of mine on hidden variables. If I understand correctly, Lochak finds that I failed somehow to allow for the effect on these variables of the measuring equipment. I will try to explain why I do not agree. The opportunity will also be taken here to comment on another refutation 2), by L. de la Peña, A.M. Cetto and T.A. Brody, and on another 3) by L. de Broglie. Yet another refutation of the same theorem, by J. Bub 4), has already been refuted by S. Freedman and E.P. Wigner 5).

Let us recall a typical context to which the theorem is relevant. A "pair of spin $\frac{1}{2}$ particles" is produced in a space-time region 3 and activates counting systems, preceded by Stern-Gerlach magnets, in space-time regions 1 and 2. The system at 1 is such that one of two counters ("up" or "down") registers each time the experiment is done; correspondingly we label the result there by A (= +1 or -1). Likewise the system at 2 is such that one of two counters registers each time the experiment is done, giving B (=+1 or -1). We are interested in correlations between the counts in 1 and 2, and define a correlation function

$$\overline{A\ B}$$

which is the average of the product of A and B over many repetitions of the experiment.

Now it would certainly be better to give a purely operational, technological, macroscopic, description of the equipment involved. This would avoid completely any use of words like "particle" and "spin", and so avoid the possibility that someone feels obliged to form a personal microscopic picture of what is going on. But it would take quite long to give such a purely technological specification. So, please accept that the words "particle" and "spin" are used here only as part of a conventional shorthand, to invoke without lengthy explicit description the <u>kind</u> of experimental equipment involved, and with no commitment whatever to any picture of what, if anything, really causes the counters to count.

Suppose that part of the specification of the equipment is by two unit vectors $\hat{a}$ and $\hat{b}$ (e.g., the directions of certain magnetic fields at 1 and 2). Then according to ordinary quantum mechanics situations exist for which

$$\overline{A\ B} = -\hat{a}\cdot\hat{b} \qquad (1)$$

to good accuracy.

Actually it is this last statement which is challenged by de Broglie. Although his paper is called "Sur la réfutation du théorème de Bell", it is not in fact concerned with any reasoning of mine. He is of the opinion that the correlation function (1) simply cannot occur for macroscopic separations, either in nature or in ordinary quantum mechanics: "Nous échappons complètement à cette objection puisque, pour nous, les mesures du spin sur des électrons éloignés ne sont pas corrélées". As regards ordinary quantum mechanics, de Broglie disagrees here with most students of the subject, and I am unable to follow his reasons for doing so. As regards nature, he seems to disagree also with experiment 6).

Now we investigate the hypothesis that the final state of the system, in particular A and B, would be fully determined by the equations of some theory if the initial conditions were fully specified. So to parameters like $\hat{a}$ and $\hat{b}$, subject to experimental manipulation, we add a list of hypothetical "hidden" parameters λ. We can take these λ to be the initial values (say just after the action of the source) of some corresponding dynamical variables. We have no interest in what subsequently happens to these variables except in so far as they enter into the measurement results A and B. But in so far as they do enter into A and B we allow fully for the effect of the measuring equipment by allowing A and B to depend not only on the initial values λ of the hidden parameters but also on the parameters $\hat{a}$ and $\hat{b}$ specifying the measuring devices:

$$A(\hat{a}, \hat{b}, \lambda)$$
$$B(\hat{a}, \hat{b}, \lambda) \qquad (2)$$

We have no need to enquire into the precise nature of this dependence on $\hat{a}$ and $\hat{b}$, nor into how it comes about, whether by the effect of the measuring equipment on the hidden variables of which the λ are the initial values, or otherwise.

Can one find some functions (2) and some probability distribution $\rho(\lambda)$ which reproduces the correlation (1) ? Yes, many, But now we add the hypothesis of locality, that the setting $\hat{b}$ of a particular instrument has no effect on what happens, A, in a remote region, and likewise that $\hat{a}$ has no effect on B :

$$A(\hat{a}, \lambda)$$
$$B(\hat{b}, \lambda) \qquad (3)$$

With these local forms, it is not possible to find functions A and B and a probability distribution ρ which give the correlation (1). This is the theorem. The proof will not be repeated here.

Lochak illustrates the way in which the output of a single instrument A depends on its setting â, as allowed for in (3), in the hidden parameter theory of de Broglie. I think this is very instructive. But more instructive for the present purpose is the case of two instruments and two particles. Then one finds that in de Broglie's theory the dependence is not of the local form (3) but of the non-local form (2). I have made this point on several occasions, in two of the three papers referred to by Lochak and elsewhere 7). It may be that Lochak has in mind some other extension of de Broglie's theory, to the more-than-one-particle system, than the straightforward generalization from 3 to 3N dimensions that I considered. But if his extension is local it will not agree with quantum mechanics, and if it agrees with quantum mechanics it will not be local. This is what the theorem says.

The objection of de la Peña, Cetto, and Brody is based on a misinterpretation of the demonstration of the theorem. In the course of it reference is made to

$$A(\hat{a}', \lambda) \quad , \quad B(\hat{b}', \lambda)$$

as well as

$$A(\hat{a}, \lambda) \quad , \quad B(\hat{b}, \lambda)$$

These authors say "Clearly, since A, A', B, B' are all evaluated for the same λ, they must refer to four measurements carried out on the same electron-positron pair. We can suppose, for instance, that A' is obtained after A, and B' after B.". But by no means. We are not at all concerned with sequences of measurements on a given particle, or of pairs of measurements on a given pair of particles. We are concerned with experiments in which for each pair the "spin" of each particle is measured once only. The quantities

$$A(\hat{a}', \lambda) \quad , \quad B(\hat{b}', \lambda)$$

are juste the same functions

$$A(\hat{a}, \lambda) \quad , \quad B(\hat{b}, \lambda)$$

with different arguments.

J. S. Bell
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1) G.Lochak - Fundamenta Scientiae (Université de Strasbourg, 1975), No 38, reprinted in Epistemological Letters September 1975, p. 41.
2) L.de la Pena, A.M. Cetto and T.A.Brody - Nuovo Cimento Letters 5, 177 (1972).
3) L.de Broglie - CR Acad.Sci. Paris 278, B721 (1974).
4) J.Bub - Found.Phys. 3, 29 (1973).
5) S.Freedman and E.Wigner - Found.Phys. 3, 457 (1973).
6) S.J.Freedman and J.F.Clauser - Phys.Rev.Letters 28, 938 (1972). A brief account is given by M.Paty - Epistemological Letters, September 1975, p. 31.
7) J.S.Bell - "On the Hypothesis that the Schrödinger Equation is Exact", CERN Preprint TH. 1424 (1971).

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14.2 F. Bonsack - Remarques à propos de l'article de G.Lochak (14.0) et de la réponse de J.S.Bell (14.1)

M. Bell et M. Lochak ayant, semble-t-il, quelque peine à se mettre sur la même longueur d'onde, je tenterai de leur en proposer une.

M. Bell admet un ensemble de paramètres, qu'il résume par la lettre λ, et une certaine distribution de ces paramètres, caractérisée par une densité de probabilité $\rho(\lambda)$. λ caractérise l'état initial du système à mesurer S au temps t_o, mais il existe une loi (inconnue) d'évolution de λ, de telle sorte qu'il suffit de connaître $\lambda(t_o)$ pour connaître $\lambda(t)$ pour $t \geq t_o$.

Cette loi d'évolution dépend évidemment de la situation expérimentale dans laquelle on mettra le système S ; celui-ci n'évoluera sans doute pas de la même manière si l'on mesure dans la direction $\vec{a}$ ou dans la direction $\vec{a}'$, mais pour chaque situation de mesure, on a une loi d'évolution déterminée de telle sorte que pour chaque valeur de λ, on a une valeur $A(\vec{a}, \lambda)$ comme résultat d'une mesure de type $\vec{a}$.

Cette fonction $A(\vec{a}, \lambda)$ permet évidemment de calculer, à partir de la distribution $\rho(\lambda)$, une distribution $\rho_{\vec{a}}(A)$ des résultats d'une mesure de type $\vec{a}$.

C'est pourquoi M. Bell peut écrire très justement: "we allow fully for the effect of the measuring equipment by allowing A and B to depend not only on the initial values λ of the hidden parameters, but also on the parameters $\hat{a}$ and $\hat{b}$ specifying the measuring devices."

Comment alors comprendre l'objection de M. Lochak, qui prétend qu'il est illégitime d'utiliser cette distribution $\rho(\lambda)$ pour calculer le résultat de la mesure, parce qu'il y a eu perturbation par l'appareil de mesure et que de ce fait la distribution a changé.

Pour la comprendre, il faut admettre, comme le postulait Bohr, que le système quantique est un système ouvert et qu'il subit une perturbation incontrôlable de la part de l'appareil de mesure.

Si la perturbation était connue, autrement dit si tout système caractérisé par un certain était perturbé, lors d'une mesure de type $\vec{a}$, d'une manière déterminée, de telle sorte que, après la perturbation, le système se retrouve dans un état qui ne dépende que de λ, il serait toujours légitime d'utiliser $\rho(\lambda)$ pour calculer la distribution des mesures $\rho_{\vec{a}}(A)$. Par contre, si pour une même mesure a et pour une même valeur du paramètre λ, plusieurs résultats A sont possibles - la distribution $\rho(\lambda)$ ne suffit plus. Dans ce cas, non seulement le système mesurant $S_{\vec{a}}$ peut évoluer différemment, c'est-à-dire donner des résultats de mesure variables, mais le système mesuré S est lui aussi perturbé de façon aléatoire et incontrôlable dans son évolution ultérieure. Car l'interaction d'un système déterminé avec un système indéterminé provoque en général une indétermination dans l'évolution ultérieure des deux systèmes.

Pour rétablir le déterminisme, il faut alors fermer le système (1), c'est-à-dire lui adjoindre, dans un supersystème, le système avec lequel il est en interaction, à savoir l'appareillage de mesure $S_{\vec{a}}$. Le résultat de la mesure n'est alors plus déterminé par λ seul, mais encore par un $\mu_{\vec{a}}$ caractérisant l'état de $S_{\vec{a}}$. De telle sorte que la distribution des résultats d'une mesure ne dépend plus de la seule distribution $\rho(\lambda)$, mais d'une distribution $\rho(\lambda,\mu_{\vec{a}})$ calculable à partir de $\rho(\lambda)$ et de $\rho(\mu_{\vec{a}})$.

MM. Bell et Lochak s'ont-ils d'accord avec cette interprétation de leurs divergences?

(1) J.L.Destouches dans "Louis de Broglie, physicien et penseur", A. Michel, Paris (1953), pp. 67-85.

Communiqué

Cher Monsieur,

En liaison avec le problème d'interprétation de la Mécanique Quantique (et en particulier avec le paradoxe EPR) je signale aux correspondants des Lettres Epistémologiques la récente parution du volume fort intéressant Quantum Physics and Parapsychology, Proceedings of an International Conference held in Geneva, 1974 (Parapsychology Foundation Inc., 29 West 57th Street, New York, N.Y. 10019). Plusieurs physiciens distingués ont participé aux exposés et aux discussions, ainsi que, notamment, Helmut Schmidt, bien connu des lecteurs de cette Revue, et que A. Koestler.

Bien amicalement,

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